Response to the reviewer

The authors would like to thank the reviewer for his/her feedback and the time he/she took to review our manuscript. Please find below point-by-point replies to the reviewer's comments. Reviewer comments are highlighted in black and author responses are in blue.

The authors improved the ms but the response of the OMZ to climate change is a crucial issue and involves a complex interaction of processes which are only partly addressed in the sensitivity experiments carried out by Lachkar et al.. As stated by the authors in chapter 4.4.2 first sentence, the study focuses on the sensitivity of OMZ to monsoon winds. That is interesting and I would suggest to adapt the abstract, introduction and conclusion to the focus of the ms.

The referee suggests to "*adapt the abstract, introduction and conclusion to the focus of the manuscript*" which is the study of *"the sensitivity of OMZ to monsoon winds"*. We believe that this is already the case as can be seen in the following statements:

In the abstract:
(lines 5-7)
"…Yet, the response of the OMZ to these wind changes remains poorly understood and its amplitude and timescale unexplored. Here, we investigate the impacts of perturbations in Indian monsoon wind intensity (from -50% to +50%) on the size and intensity of the Arabian Sea OMZ, and examine the biogeochemical and ecological implications of these changes…"

(lines 18-20)
"We conclude that changes in the Indian monsoon can affect, on longer timescales, the large-scale biogeochemical cycles of nitrogen and carbon, with a positive feedback on climate change in the case of stronger winds."

In the introduction (p3, lines 31-34):
"…Here we address these questions and explore the mechanisms by which the Arabian Sea ecosystem responds to monsoon wind changes using a regional eddy-resolving model. We examine how idealized changes in summer and winter monsoon wind intensity affect the productivity and the volumes of hypoxic and suboxic water in the Arabian Sea and explore the biogeochemical and ecological implications of these changes…."

In the method section (section 2.2, p5, 19-22):
"Although these runs explore different wind perturbation scenarios, they are highly idealized by nature and are not intended to mimic future projections or realistic future trajectories, but rather aim at exploring the sensitivity of the Arabian Sea OMZ to monsoon wind intensity changes and improving our understanding of the key mechanisms that control the OMZ response and its timescales."

In the results section (section 3, p10, 26-27):
"To explore the sensitivity of the Arabian Sea ecosystem to changes in the intensity of monsoon winds, we consider various scenarios of idealized wind perturbations."

In the discussion section (section 4.4.2, p21, lines 30-31, p22, line 1):
"The primary focus of this study is the sensitivity of the Arabian Sea OMZ to monsoon wind changes and its response timescale. This justifies the use of highly idealized wind perturbations, as our simulations are not intended to mimic realistic future changes but rather to deepen our understanding of the key mechanisms at work and their potential implications."

In the conclusions (section 5, p23, lines 5-6):
"A set of coupled physical biogeochemical simulations of the Arabian Sea ecosystem reveals a tight coupling between the intensity of the summer monsoon wind and the size and intensity of the Arabian Sea OMZ".

At no point in the manuscript we claim the paper addresses the question of the response of the OMZ to climate change which is a much more complex problem that involves perturbations that are not covered in the study (e.g., increased warming and stratification, changes in large scale ventilation, changes in biological productivity unrelated to changes in the winds (for example due to atmospheric deposition of nutrients, etc…)).

Considering that Lachkar et al. are interested in the contemporary and even future ocean in addition to glacial also results obtained from Holocene records should be included in the discussion. First of all because of the similar boundary conditions and secondly also because results from the Holocene record contradict the presented interpretation of the model results.

Our study explores how the Arabian Sea OMZ responds to changes in monsoon winds and the timescales and the mechanisms that drive this change. It is not about reproducing the past evolution of the OMZ or its future trajectory. This is made explicit in p5, line 20 of the revised manuscript, see our statement: "...they are highly idealized by nature and are not intended to mimic past conditions from paleoclimatic reconstructions or future trajectories" (see also p21, line 31).
In order to simulate the conditions of early Holocene and explain the trends the reviewer is referring to, one would need to use realistic atmospheric conditions (not only for winds, but also heat and freshwater fluxes) and lateral boundary conditions of temperature, salinity, nutrients and oxygen that prevailed during this period, as well as a realistic representation of the Red Sea and the Gulf at that time (probably partially or entirely closed because of the much lower sea level). This is an entirely different question that lies far beyond the scope of this study.
However, following the suggestion of the referee we have added to section 4.2.3 a brief discussion of the OMZ changes during the Holocene and how to reconcile our results with the recent findings by Gaye et al. (submitted to Biogeosciences discussions) and Das et al. (2017). Please see our detailed response to next referee's comment and p20, lines 7-12 in the revised manuscript.

For example, looking from today back into the past (the last 10000 years) decreasing d15N values within sedimentary records (especially from center of the OMZ in the north and east) correspond with decreasing burial rates of organic matter and an increasing summer monsoon strength. This lead to the conclusion that a strengthening of the summer monsoon weakens the OMZ due to associated changes in the ocean's circulation and ventilation during the Holocene. Despite critics of the authors on global models recent results obtained from a global ocean circulation model support this conclusion by showing an increasing volume of oxygen depleted water (OMZ) associated with nearly constant productivity and weakening of the summer monsoon during the Holocene (from the past to the present).
See Gaye et al. 2017 https://www.biogeosciences-discuss.net/bg-2017-256/. and older references therein.

We do not believe there is a contradiction between our results and the findings of the study the reviewer is referring to. Our study confirms the strong link that exists between the strength of SW monsoon winds and the intensity of the Arabian Sea OMZ and denitrification levels in agreement with a large number of paleoceanographic studies that suggest higher OMZ intensity and elevated denitrification rates during warm periods (with stronger SW monsoon) and weaker OMZ and reduced or absent denitrification during cold periods (Altabet 1995, Altabet 1999, 2002, Reichart et al 1998, Schulte et al. 1999). However, we do not claim that the Arabian Sea OMZ intensity is completely determined by the intensity of the monsoon winds. Instead, our study shows that if only monsoon winds are perturbed (with the assumption that everything else is kept constant, which seems not to be the case during the late Holocene), then the OMZ and denitrification will strongly increase in response to monsoon intensification. We also recognize among the study caveats the fact that "we considered the effects of monsoon changes in isolation" (see p23, lines 1-3).

The fact that an intensification of the OMZ may have coincided at certain point in time (middle to late Holocene: ~4200 years BP onwards according to Das et al 2017) with a weakening of the SW monsoon does not lead to the conclusion that a weakening of the monsoon causes an intensification of the OMZ (or a strengthening of the summer monsoon weakens the OMZ as put by the referee). Instead, this means that the OMZ intensity is not entirely driven by the SW monsoon intensity, but can be affected by other factors (such as changes in large-scale ventilation and fluctuations in exchange with Red Sea and Arabian/Persian Gulf) as suggested by some previous studies (e.g., Pichevin et al, 2007, Boning and Bard 2009, Das et al, 2017) and already acknowledged in our manuscript (see p20, lines 1-4).

We note that the response timescale to monsoon wind changes can be relatively short (decades to centuries) while OMZ fluctuations involving large-scale ventilation changes are associated with much longer timescales (centuries to thousands of years). Therefore, this suggests that abrupt changes in denitrification and OMZ intensity recorded in marine sediment data (e.g., during Dansgaard-Oeschger events or Heinrich events) are more likely to result from monsoon wind changes than changes in ventilation by large-scale circulation or marginal seas.

To avoid any misinterpretation of our results, we further stress the potential role of ventilation changes in the discussion section (4.2.3). More specifically, we state: *"Besides monsoon strength, some studies have linked past OMZ intensity changes to changes in the rate of formation and subduction of oxygen enriched Subantarctic Mode Water (SAMW) and Antarctic Intermediate Water (AAIW) in the Southern Ocean in association with Atlantic Meridional Overturning Circulation (AMOC) fluctuations (Pichevin et al, 2007; Boning and Bard, 2009)."* (please see p20, lines 1-4 in the revised manuscript).

Regarding the Holocene, we have added: *"In the Holocene, large-scale ventilation changes may have played an important role together with the fluctuations in monsoon intensity as suggested by recent studies (Das et al, 2017, Gaye et al., submitted to Biogeosciences discussions). For instance, recent paleo reconstructions by Das et al. (2017) suggest an intensification of the Arabian Sea OMZ from the middle to late Holocene despite a weakening of the SW monsoon winds. These authors hypothesize that the recent (~ 4000 years BP onwards) decline in oxygen at depth may have resulted from a cut off of the Arabian Sea OMZ from the oxygen enriched AAIW and SAMW."* (Please see p20, lines 7-12 in the revised manuscript).

Furthermore the authors are correct figure 7b shows in line with observations the denitrification peak immediately below the mixed layer. Denitrification at depth between 400 and 1300 m with a small secondary peak at 500 m is unsupported by observations. This means there are two peaks: one shallow one supported by observations and another deep one which is unsupported by observations.

We would like to highlight that what is shown in Fig7b is domain integrated denitrification (in Gmol per meter of depth) as a function of depth. This cannot be compared to individual profiles from observations. Indeed, because of the very limited number of available direct measurements of denitrification rates, especially at depths below 300m, no equivalent can be derived from observations. This is particularly true because of the patchiness of denitrification in space and time that results in strong heterogeneity in denitrification profiles.

Our domain-integrated denitrification is maximum between 100 and 300m, but shows a much (a factor 6 or 7) weaker secondary maximum between 500 and 800m. This secondary maximum can be explained by the fact that the (potentially denitrifiying) suboxic area is largest at this depth (see Fig7a) and denitrification is still detectable in our model at this depth, although occurring at more than an order of magnitude smaller rate (on per unit of volume basis) than in the upper 200m.

We searched the relevant literature and found no evidence pointing towards a complete shutdown of denitrification below 400m as can be inferred from the reviewer's comment. Actual measurements of denitrification rates (incubation) have been few, but in these few studies, denitrification was found at all sampled depths (down to 350m for Devol et al, 2006 and down to 400m for Bulow et al (2010). In Devol et al (2006), there was no clear depth structure found between 150 and 300m. In Bulow et al (2010), denitrification rates were largest in the layer between 150-200m, but significant denitrification was found down to 400m. No samples were taken below this depth (400m) in any of the two studies.

Most previous estimates of denitrification in the Arabian Sea are based on indirect methods involving the concept of nitrate deficit (a pool of missing nitrate resulting from denitrification) that involves stoichiometric ratios between nitrate, phosphate and oxygen together with estimates of residence times of denitrifying water masses. An alternative way to express nitrogen anomalies is through nitrogen gas excess. The large uncertainties associated with these assumptions result in large uncertainty in the derived denitrification estimates that vary by more than a factor four between the different studies (Naqvi, 1987 ; Mantoura et al., 1993; Howell et al., 1997 ; Codispoti et al., 2001).

Several previous studies using indirect methods suggest the occurrence of occasional and weak (but still detectable and non negligible) denitrification well below 400m. For instance, evidence from Morrison et al (1999) shows important variability in vertical profiles of nitrate deficits estimated at 4 JGOFS stations in the Arabian Sea in 1995 with significant nitrate deficits reaching as deep as 1000m in one station. Naqvi (1994) shows significant nitrate deficits down to 1000m. He further notes: “a deeper (700-1200m) denitrifying layer may also develop occasionally, probably due to advection of nepheloids layers from the continental margins”.
Bange et al. (2001) has linked the secondary peak in N_2O between 800-1000m in the Arabian Sea to occasional denitrification. In Codispoti et al (2001) and Devol et al, (2006), both nitrate deficit and nitrogen excess (indicative of denitrification) are found to be maximum around 200-400m, but remain significant down to 1000-1500m and below.

Because nitrate deficits (and nitrogen excess) reflect denitrification integrated over time, accumulation of nitrite, an intermediate in the process of denitrification, has conventionally been used as an indicator of active denitrification. Most available nitrite profiles in the Arabian Sea show high concentrations between 200 and 400m (secondary nitrate maximum: SNM) and very low concentrations below.
Yet, Naqvi (1994) shows significant nitrite concentrations reaching down to 500m in the northern Arabian Sea. In Morrison et al (1999) the SNM is shown to reach as deep as 600m. In another study by Brand and Griffiths (2008), measurements of nitrite from a series of cruises between March and October 2003 in the north east Arabian Sea shows significant nitrite concentrations down to 1000m with a maximum at around 600m. Finally, Sokoll et al (2012) measured significant nitrite concentrations down to 600m in the northeast Arabian Sea off the coast of Pakistan.

Finally, a recent study by Lam et al. (2011) shows a decoupling between nitrite concentrations and measured denitrification rates between the Omani shelf region and the central Arabian Sea. These authors show that the accumulation of nitrite results from a production of nitrite (through denitrification) that exceeds its consumption rate (due to denitrification and anamox) and conclude that the absence of nitrite (or its presence at very small concentrations) does not necessarily imply an absence of denitrification. This questions the traditional use of nitrite as an indicator of active denitrification.

In summary, the double peak structure that characterizes the depth profile of the domain-integrated denitrification in our model can be explained by: 1) the very high denitrification rates in the top 200m and 2) the presence of weak denitrification at depth across a very wide area occupied by suboxic waters (500-1000m). Our model results are not conflicting with data based evidence as suggested by referee, as the presence of weak denitrification at depth (>400m) has been reported in several previous studies as shown earlier. Furthermore, the total water column denitrification in our model (18.5 Tg N/yr) lies within the range of previously published data-based estimates that range from 10 to 44 TgN/yr (Naqvi, 1987 ; Mantoura et al., 1993; Howell et al., 1997 ; Codispoti et al., 2001).

For more clarity, we added in the revised manuscript a statement that explains the secondary maximum in integrated denitrification profile shown at depths between 500 and 800m. Please see caption of Fig. 7 where we have added the following text: *"Note the presence of a weak secondary maximum in domain-integrated denitrification between 500 and 800m in all simulations. In the control simulation, this can be explained by the fact that at this depth range the area occupied by (potentially denitrifying) suboxic water is largest and weak denitrification is still present although at an order of magnitude weaker rate (on per unit of volume basis) than at 200 m."*

By increasing wind speeds the supported peak decreased and the unsupported peak increased. Since the latter overcompensated the first the authors concluded an intensifications of the summer monsoon winds deepens the zone of denitrification and increase denitrification rates. To base a conclusion on the intensification of an unsupported peak is to my understanding questionable. Ignoring the response of the unsupported peak would show that stronger summer monsoon winds lower denitrification, which is in line with the Holocene d15N records and the global ocean circulation model. These aspects should also be considered in the discussion and conclusion.

The increase of denitrification at depth and its weakening near the surface is a direct consequence of the expansion of the suboxic area at depth and its shrinking near the surface (see Fig7a). This is a consequence of enhanced upper (0-200m) ocean ventilation and increased biological consumption at depth (depth >200m) as evidenced by the oxygen budget presented in Figure 9. We cannot "ignore" the deep OMZ response to make the results follow a scenario likely involving very different boundary conditions (e.g., potential changes in the oxygen lateral boundary conditions during the Holocene).

Some more minor comment refer to the discussion about sinking speeds and respirations rates, light limitation, classification of zones. If it does not matter whether one uses high sinking speeds and respirations rates or low sinking speed and low respiration rates why do not stick to observation in order to avoid such discussions.

The values of sinking speed and remineralization rates used in the model are the default values used in the Gruber et al., (2006) NPZD model.

Please explain in more detail how light limitation was identified and translated into reduced carbon export rates and the why unused nutrient were not consumed somewhere else?

Our reasoning refers to Sverdrup's critical depth (SCD) hypothesis and the observation that winter phytoplankton growth can be limited by the amount of available light resources. A deepening of the mixed layer associated with an increase in upper ocean turbulence moves rapidly phytoplankton through the mixed layer where average available amount light can decrease if the turbulent mixed layer gets deeper than the euphotic zone (or more precisely the compensation depth defined as the depth where the phytoplankton loss terms such as mortality, grazing and respiration compensate photosynthesis). For a review of SCD see Franks (2015).

Following the reviewer's suggestion we have added a few statements to further clarify how the deepening of the winter mixed layer could increase the light limitation and limit productivity increase. More specifically, we have added the following text to section 3.1 (page 12, lines 5-9): *"Indeed, winter turbulent mixed layer deepens in the northern Arabian Sea by up to 20-25m and penetrates below the euphotic zone (1% light depth) at 65-70m. This increases the average exposure of phytoplankton to light-limited conditions, thus potentially limiting the net growth (i.e., gross photosynthetic rate minus loss terms due to mortality, grazing, sinking and respiration) over the water column, and hence reducing the potential biomass and productivity (Franks 2015)."*

According to fig 7a I would suggest to define upper OMZ from below the mixed layer to approximately 375 m (supported denitrifying zone) and the mesopelagic zone between 375 and 1800 m (unsupported denitrifying zone).

We assume the reviewer is referring to the vertical layers (epipelagic 0-200m, mesopelagic 200-1000m and bathypelagic >1000m) used to highlight the OMZ response timescales (Fig. 8) and O_2 budgets (Fig. 9). Our use of the three layers is intended to highlight how the OMZ response timescales vary as a function of depth. The response is fastest in the upper (0-200m) ocean and slowest in the deep ocean (>1000m). Changing slightly the definition of the 3 layers won't change the conclusion, as the increase in the timescale of the OMZ response translates the differences in ventilation (circulation) timescales that exist between the surface, the intermediate and the deep ocean. Furthermore, the chosen layers have direct ecological and biogeochemical implications as the top (0-200m) layer corresponds roughly to habitats of epipelagic fishes while the mesopelagic layer (200-1000m) is the region where 90% of oxygen biological consumption occurs (Robinson 2010). Therefore, we prefer to stick to our definition of the three vertical layers.

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